THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison 6

DEPARTMENT OF GENETICS

May 5, 1953

Professor E. L. Tatum
Department of Biology
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California

Dear Ed:

I'm sorry we did not get to see you during your visit, but perhaps it's just as well, since Esther and I were both half-expired with colds at the time.

You must be as annoyed as I am about the inexcusable delays in the AAAS publication. I am glad that you concur in not wanting to revise: we could do that sort of thing indefinitely. If I may confess one additional slight displeasure, to which I may or may not be entitled, it is that you presented me with a fait accompli on the reversal of authorship. I can understand well enough why you should exercise this prerogative, and will acquiesce in it, but would have appreciated the opportunity to try to dissuade you from it. Let it stand, but if you should change your mind during the next few years while this thing is in press, I will hasten to insist on the restoration of the original order.

There are two scientific matters about which I wanted to ask for some information. The first is 58-161 (perhaps we've gone over this before): do you have any cultures of 58-161 or proximate derivative of it, for which there is any hope of the retention of the biotin requirement? Everything I have is definitely B+ (with 58 biotin-dependent as control), and Phyllis Fried was unable to so much as repeat the isolation of B- recombinants per table 4 of the 1947 Genetics paper. I cannot find, in notes of experiments at Yale, explicit tests of the nutrition of 58-161, except that it was noted that platings on methionine agar were quite turbid, so that B- could not be used as a selective marker. The most recent experiment in which B- recombinants were detected was 3/9/47..I assumed all along that either agar or methionine was furnishing or sparing biotin. ~~While this is doubtless true to some extent.~~ While these are both true, to some extent, they do not explain the fact, meanwhile concealed, that "58-161" has lost its biotin requirement. Would it be too much to ask what specific nutritional tests you may have made on the biotin-requirement of 58-161 since its isolation? To avoid further confusion, we intend to describe our present culture, B+M- as W-6, an inadvertent, spontaneous reversion of 58-161, B-M- (possibly no longer availabl

The second matter concerns a paper that Cavalli & I have been writing on the genetics of resistance, for the Rome Congress. (We probably will not be there in person, and Cavalli will read it). I do not know if there would still be time to include this reference, but I recall your once remarking on a brief study of resistance to valine or acetyl-valine. Has this ever been written up in any form? As I remember there were some possibilities in these as markers. Any details you could furnish, either for reference or for my own curiosity would be appreciated.

Are you going to the Congress?

Sincerely,

Joshua

P. S. Note: authorship of Lederberg & Lederberg [Lysogenicity] is E.M.L. and J.L. rather than reverse. This can probably wait for proof by when we should also have the pages.

JL